

Mr. Morgan's physiological definitions of reflex action, instinct, and intelligence. If we want such a definition it must be made independently of any zoological classification, and with exclusive reference to the point whether the adaptive action requires for its performance the operation of the higher nerve-centres—a point which can only be determined by vivisectional experiment. In other words, on the side of objective psychology the only distinction that can be drawn between a reflex and an instinctive action, is as to whether the action can be performed by the lower nerve-centres alone, or requires likewise the cooperation of the higher nerve-centres. And this is just what we should expect to find to be the case on the objective side if, as I have endeavoured to show, the one peculiarity which distinguishes actions classed as reflex from actions classed as instinctive, consists in the latter exhibiting in their performance a mental or conscious element which is not exhibited in the former.

Now, if the *raison d'être* of the term "instinct" is thus to denominate a class of adaptive actions in which there is a subjective, or rather let us say an ejective element, I cannot see that anything but confusion is to be gained by forcing this term into objective implications. Were any term needed to designate the neurosis of instinctive action, it would be far better to coin a new one than thus to abuse an old one. I am fully sensible of the difficulty which often arises in deciding whether a particular action should be assigned to the instinctive or to the reflex class; but, as I observe in "Mental Evolution in Animals," "this difficulty does not affect the validity of the classification any more, for instance, than the difficulty of deciding whether *Limulus* should be classified with the crabs or with the scorpions affects the validity of the classification which marks off the group Crustacea from the group Arachnida."

For the rest, Mr. Morgan's criticism on my psychological definition of instinct hangs entirely upon his previous criticism as to the possibility of a science of comparative psychology, and as I have already endeavoured to answer the latter, I need not go over the same ground again by answering the former. There are only two points raised by his paper to which this general answer does not apply, and with these, therefore, I shall conclude.

The first of these two points is a charge of inconsistency. My critic observes that, after having said "it is enough to point to the variable or incalculable character of mental adjustments as distinguished from the constant and foreseeable character of reflex adjustments," I go on to define instinctive actions as mental adjustments which are nevertheless of a constant and foreseeable character. Now I think, if any one will read my chapter on "The Criterion of Mind," he will see that this apparent inconsistency is not a real one. It would be a real one if the passage above quoted referred only to this and that particular action of an animal, apart from all the other actions of the same animal, which, according to my criterion of mind, are competent to inform us whether or not the animal in question is a *choosing* and *perceiving* animal. But the passage quoted refers to the whole constitution of an animal so far as we can know it by observation of activities, and therefore the question whether this or that particular activity is to be regarded as mental or non-mental (instinctive or reflex) requires to be answered by all that we learn concerning the other activities of that animal. If none of its activities are other than those of a constant and foreseeable character, we have no reason to suppose that it is a *choosing* or *perceiving* animal; but if some of its other activities are indicative of choice and perception, our knowledge of this fact must be allowed due weight in any attempt that we may make at classifying this or that particular action as reflex or instinctive. The case, in short, is just the converse of that which I thus state in the chapter referred to:—"Many adjective actions which we recognise as mental are, nevertheless, seen beforehand to be, under the given circumstances, inevitable; but analysis would show that this is only the case when we have in view agents whom we already, or from independent evidence, regard as mental."

The second point to which I have referred as the only one that now remains for me to consider, is to the effect that I have mistaken "Mr. Spencer's position with regard to the 'very subordinate importance of natural selection as an evolving source of instinct,' and with regard to the question of 'lapsed intelligence.'" Here I can afford to be brief, inasmuch as any one who cares to do so can compare my interpretation of Mr. Spencer's writings with the passages in those writings to which I refer. It seems to me perfectly clear that, although both the principles in question are alluded to by Mr. Spencer, neither of them holds the same pro-

minence in his theory of the development of instincts from reflex action as they hold in the theory of Mr. Darwin.

In conclusion, I trust Mr. Morgan may feel that, in writing this somewhat elaborate reply to his criticism, I am marking as emphatically as I can my sense of its ability. And if the general effect of this discussion is to show that the phenomena of instinct present peculiar difficulties to any attempt at a fundamental analysis, I should like no less emphatically to express my conviction that such an analysis is not to be facilitated by closing our eyes upon the entire class of phenomena to which alone the word is applicable. We may, of course, abstain from any attempt at such analysis, and devote our attention exclusively to the physical as distinguished from the mental side of the subject. Only in this case we may not speak of *instinct*.

GEORGE J. ROMANES

### "Mental Evolution in Animals"

MR. ROMANES' comment on my communication in NATURE of February 7 (p. 335) is not quite satisfactory. I do not suppose that he has any spite against my skate; but as he does not know me, and did not see the incident in the Manchester Aquarium, I think it is very possible that he may have been naturally predisposed to underrate the significance of the story. I do not admit that I can be reasonably blamed for saying that a repetition of the conditions would have been useful, if possible, while at the same time pointing out that the result would not necessarily have settled the question. Test experiments are always useful, even if they do not settle the main question. Mr. Romanes' terrier story was not necessary to make clear what he means by "accident," and there is no analogy between it and my skate story. In one case a trained, or at least tamed, dog did as he was told, and the conditions of success were prearranged; in the other, a fish spontaneously did something for his own advantage. As for the fish smelling the food, this does not harmonise with the circumstances as I described them, and had Mr. Romanes seen the incident I do not think this explanation would have occurred to him; the whole series of actions was too rapid, and had too much the appearance of co-ordination. The propulsion of the food into the ready mouth was the work of an instant. Had the mouth not been ready, as the cricketer's bat is the instant the ball leaves the bowler's hand, the morsel would have been missed. Finally, Mr. Romanes tells us ("Animal Intelligence," p. 351) that the bear observed by Mr. Hutchinson was a Polar bear. Now this species is "almost marine in its habits." It lives upon seal-flesh and also upon dead meat which it finds floating in the water. It is not infrequently cast adrift on an ice-floe or an iceberg. It is therefore not at all improbable that the method of fishing described may be an instinct developed hereditarily. The fact that two bears behaved in precisely the same manner strengthens this supposition. Mr. Darwin does not say whether the bear observed by Mr. Westropp in Vienna was a Polar bear or not, but he observes that the action in question "can hardly be attributed to instinct or inherited habit," as it would be "of little use to such an animal in a state of nature." It seems to me that such action would be very useful to Polar bears in a state of nature.

Manchester, February 11

F. J. FARADAY

### The Remarkable Sunsets

AT the present stage of the discussion upon the "green sun" and rosy sunsets it seems to me it would be well to recall attention to a few facts, for there seems to be a tendency on the part of some correspondents to allow imagination to carry them beyond the region of fact into that of fancy. First, then, I would point out that my observations show conclusively that at the time of the green sun there was an altogether abnormal amount of moisture in the upper regions of the atmosphere, while the ordinary hygrometric observations showed the air near the ground to be comparatively dry. I have studied the rain-band spectrum almost daily for the last six or seven years, and I have never before known such a long continuance of the heavy rain-band in a comparatively clear sky—a sky in which there was only a light haze. At sunset and sunrise the intensity of the bands was such as I have before seen only from an altitude of some six or seven thousand feet, and even then rarely. In this connection it may be well to point out that the spectrum as observed by Mr. Donnelly (NATURE, vol. xxix. p. 132), though, as remarked by Mr. Lockyer, resembling that observed here in

some respects, yet differed from it in some important points. The "low sun-bands" appeared weak rather than strong, partly perhaps by contrast with the great intensity of the rainband, and the rainband itself was easily divided into lines, of which eight are recorded in my note-book as being seen with a one-prism spectroscope. The band between *b* and *F*, observed by Mr. Lockyer, was also seen here, and was found to be one ascribed to aqueous vapour, W.L. 504. A spectrum almost in all respects similar to that observed here can be seen by any one who will examine the absorption produced by a *small* cloud passing over the sun as seen with the spectroscope, having a lens in front of the slit. The contrast with the bright spectrum of the sun shows the general absorption in the red very clearly, and if the sun be near the horizon the other bands will be, in most cases, fairly well seen.

It is worth noting that we have had an unusually early and heavy monsoon, ushered in by a remarkable thunderstorm and followed by a period when the spectrum showed an abnormal freedom from vapour, the rainband at times being quite invisible. During this latter period we have had beautiful rosy after-glows, the sunlight being apparently reflected from thin, almost invisible, cirrus clouds.

If the presence of dust can be proved, these phenomena, as I previously indicated, can be readily explained in accordance with the facts so beautifully illustrated by Mr. John Aitken (*Trans. R.S.E.*, vol. xxx. p. 337), for the dust particles would condense moisture in the upper parts of the air, and we would have a light haze, such as was observed here, not sufficiently dense to cause actual clouds, but deep enough to give the special absorption effects, while the dust itself would assist in producing the general absorption.

Against the idea of Java dust, however, have to be set a number of facts of which the following are a few:—The maximum phase of greenness was on the same day (September 10), all over Ceylon and South India, and as far west as long. 64° (at sea). The green sun was not seen at Rangoon nor at the Andaman Islands, though at the latter place the sounds of the eruption were heard. The first rain that fell here afterwards was subjected to careful microscopic analysis, and showed no trace of volcanic dust. The phenomenon reappeared on September 22.

For my own part I think there is strong evidence that the influence of the Javan eruption was an electrical one, and that that was not necessarily propagated by the actual transference of matter. Mr. Whympers's very interesting letter is of course by no means conclusive as regards the effects of dust, for it is, I believe, regarded as virtually proved that the mere existence of dust in large quantities in volcanic ejecta proves the presence of an abundance of water vapour.

C. MICHIE SMITH

P.S.—There is a misprint in my letter to Sir William Thomson which, as I have seen it twice quoted, ought to be corrected. It is in vol. xxix. p. 55, line 8, which should read: "After the electricity had gone to *negative*."

C. M. S.

The Christian College, Madras, January 23

SINCE the end of October, when I first observed an unusual red glow for a considerable time after sunset, I have been a close observer of the atmospheric phenomena so fully described by your correspondents. For some time past they have appeared with little of their former brilliancy, until the evening of the 7th inst., when there was a remarkably fine display, equaling in many respects those of December. Of this I shall particularly mention but one feature which I had seen three times previously, but never displayed with such intensity and clearness of definition. At 5.30, when the after glow was at its maximum, a lovely crimson arc appeared opposite it in the eastern horizon, in every respect as described by Mr. Divers in his letter dated from Japan, which appeared in NATURE of January 24 (p. 283). I may remark that I have observed here, from November 10 to this date, but latterly with much diminished intensity, every one of the phenomena he so graphically describes.

A. C.

Roscommon, February 11

### "The Indians of Guiana"

IN the notice of Mr. Im Thurn's work on the Indians of Guiana, in the current volume of NATURE (p. 305), Mr. Tylor writes: "What is still more curious is that the rude method of

making thread by rolling palm or grass fibre into a twist with the palm of the hand on the thigh may be commonly seen in Guiana, although the use of the spindle for spinning cotton is also usual." As such a fact appears to be curious to so eminent an anthropologist as Mr. Tylor, it may be of interest to some of your readers to learn that this mode of twisting fibres is still by no means uncommon in India, though spinning must there have been familiar to the natives for unnumbered generations. I have frequently seen Hindus of various castes twist a mass of jute-fibre into a compact and firm rope of considerable length, between the palm of the hand and the inside of the thigh, and by the same means they will frequently produce long pieces of strongly coherent twine when the need for it arises. From my experience, which, though confined to a small geographical area, comprehended an acquaintance with both Hindus and Mohammedans imported into the tea-districts from almost every part of British India, I should suppose that this custom of twisting fibres into rope and twine is universal throughout the country, though doubtless it is resorted to rather as a makeshift than as a regular mode of manufacturing twisted cords. That such a means should be resorted to by the wild tribes of the north-eastern frontier is by no means strange, though these have acquired not a little skill in spinning and weaving cotton, but that so primitive a method should still prevail amongst peoples so highly cultured as the Hindus and Mohammedans of India often struck me as remarkable.

While noticing Mr. Tylor's interesting article, I cannot refrain from questioning the justice of the supposition that pile-dwellings on the land are due to the "survival of the once purposeful habit of building them in the water." That in New Guinea such is the case there can be little doubt, as Dumont d'Urville and Mr. Wallace, as well as Prof. Moseley, have remarked. And that Mr. Im Thurn's supposition with regard to the natives of Guiana is also correct there can hardly be a doubt. But these two cases scarcely seem to me sufficient upon which to generalise, even when added to Prof. Moseley's pretty and ingenious view as to the origin of the Swiss chalet. As has been pointed out to me by my friend Mr. W. E. Jones, F.R.I.B.A., Lecturer on Architecture in the Bristol University, a somewhat similar development of single-storied into two-storied dwellings is to be traced in the stone buildings as well as in the less substantial dwellings of Western Asia, between the twentieth and the twelfth centuries B.C., and though of course it is not impossible, it certainly seems improbable that a race of ancient lake-dwellers should have perpetuated on sandy plains a practice which must altogether have ceased to be useful long before it reached a region so far removed from its original home. And indeed it seems to me that pile-dwellings may be observed in localities in which it is scarcely possible that the practice could have originated in lake-dwellings, or in any dwellings of any sort erected in water, whether fresh or salt. I allude more particularly to the raised dwellings of the Nagas, Kukis, Cacharis, Khasias, and other hill-tribes of the north-eastern frontier of India, in the midst of which I lived for several years. That these people should ever have dwelt so near the sea that they acquired the habit of erecting pile-dwellings therein seems to me highly improbable when it is remembered that their racial and linguistic affinities place them undoubtedly in that great Mongolian group of which the Thibetans and Burmese are examples; and that therefore they may be regarded as immigrants from more Eastern Asia, rather than as tribes which have been gradually driven back from the Bay of Bengal by the encroaching civilisation of the Hindus. Nor does it seem probable that their pile-dwellings were originally erected in lakes amongst the hills, for in fact the lakes nowhere exist. There are indeed extensive *bheels* or marshes, which during the rainy season sometimes contain a good deal of water. But these *bheels* are, during at least a portion if not the whole of the year, so pregnant with fever and ague that I cannot believe that they were ever employed, as were the lakes of Switzerland and Italy, for the protection of the habitations of man. Yet these north-eastern frontier tribes for the most part build their houses upon piles. These are generally of bamboo, and so of course are very perishable, but occasionally small timber is employed. The floor or platform (of coarse bamboo matting) is seldom raised more than from twenty-four to thirty inches above the ground, though, if my memory serves me, I have occasionally seen it raised as much as between six and seven feet. Beneath this platform a good deal of lumber generally accumulates, and the poultry and pigs frequently congregate for shelter, but I think I never saw an